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XXIII. On the Degree of Salubrity of the common Air at Sea, compared with that of the Sea-shore, and that of Places far removed from the Sea. In a Letter from John Ingen Houfsz, M. D. F. R. S. to Sir John Pringle, Bart. F. R. S. Dated Paris, Jan. 22, 1780.

Read April 24, 1780.

SIR,

AS you had recommended to me the examination of the air at sea by the nitrous test, I followed your advice in my return to the Continent in the beginning of November last: and I embraced that opportunity with the more eagerness, as I knew that you had given credit to the account of several consumptive people having recovered their health by going on sea voyages, after the common means for curing that distemper had failed.

I was in hopes likewise to find in this inquiry, a confirmation of what you conjectured in your Anniversary Discourse in the year 1773, viz. that great bodies of water, such as seas and lakes, are conducive to the health of animals, by purifying and cleansing the air contami-
nated

nated by their breathing in it: so that the salutary gales, by which this infected air is conveyed to the waters, and by them returned again to the land, though they do rise now and then to storms and hurricanes, must nevertheless induce us to trace and to revere in them the ways of a beneficent Being, who, not fortuitously, but with design, not in wrath, but in mercy, thus shakes the waters and the air together, to bury in the deep those pestilential effluvia which the vegetables upon the face of the earth are insufficient to consume.

I was not without hope, that such experiments might tend to throw a new light upon the cause of that almost universal effect of the sea air, to wit, its increasing the powers of life, and giving a keener appetite by hastening the digestion of food.

I shall now give you an account of the experiments I made in consequence of your suggestion, in the same order as they were made; and beg you to present them to the Royal Society, if you think them worthy the attention of that learned body.

I must first, however, premise, that as I wrote this paper in noisy inns, on ship-board, and places little adapted to philosophical application, I hope you will make some allowance for the inaccuracies which you may

find in it. I began my experiments at Gravesend, where I was obliged to wait two days for a favourable wind. I found the air of that place, on Nov. 1, of a tolerable good quality, as one measure of it with one of nitrous air occupied, in several trials, about 104, or one measure and $\frac{4}{100}$ of a measure; so that I took it to be nearly of the same quality as the air of London.

The ship in which I went from London to Ostend happened to be becalmed about two or three miles from shore, in the mouth of the Thames, between Sheerness and Margate. The weather was very agreeable, warm, and the sun shone very bright, on the 3d of November. I was provided with a travelling apparatus, made on purpose by Mr. MARTIN, the whole of which was packed up in a box about ten inches long, five broad, and three and an half high. The glass tube or great measure, which was sixteen inches long, and divided in two separate pieces, lay in a small compass, and could be put together by brass screws adapted to the divided extremities. Instead of a water trough, such as is used commonly, I made use of a small round wooden tub, which I found on board the ship, and which I filled with sea-water, fixing to the edge of this tub, by means of a screw, the brass funnel, through which the air was to be let up into the glass tube.

After

After having exercised myself during some time in performing the experiment in a water tub much too small for the purpose, I at last acquired a habit of doing it tolerably well. I then began to make my experiments regularly at about eleven o'clock; and I have the pleasure to inform you, that I found the sea air at the place indicated of a superior purity to any common air I ever met with since the month of June last (when I began to engage in the course of experiments which have afforded me the materials of my work lately published upon *Vegetables*) either in my country retirement, or in London. In six different trials made one after another in the short manner described in my book, p. 278. *et seq.*^(a) I found, that the two measures of air (one of common and one of nitrous air) occupied from 0.91 to 0.94; which difference in the result, though but small in itself, was owing to the disadvantage of not having a vessel deep enough to move the glass tube in with ease, for the purpose of mixing the two airs together, and to my not having yet acquired the habit of using the portable apparatus.

I tried also the air of this spot in the manner used by Abbé FONTANA, which I have described in my book,

(a) It consists in letting up into the glass tube one measure of common air, and after this one measure of nitrous air, and shaking the tube forcibly in the water trough just at the moment the two airs come into contact with each other.

p. 155^(b); the result of which trial was, that after the first measure of nitrous air was let up, the column of both airs occupied 1.86, or one measure and $\frac{86}{100}$ of a measure; after the second measure of nitrous air was let up, the bulk of both airs occupied 202, or two measures and $\frac{2}{100}$ of a measure: and after the third measure of nitrous air was let up, the remaining bulk of both airs occupied 2.96, or $\frac{96}{100}$ of a measure: so that the remaining bulk of both airs employed in the experiment (*viz.* two measures of common and three of nitrous air, each making 100 sub-divisions of the glass tube) amounted to two measures and $\frac{96}{100}$ of a third measure, or to 296 sub-divisions, which being subtracted from the five measures, or from the 500 sub-divisions employed, the remainder was 2.04, which was exactly the quantity of both airs destroyed.

Give me leave, before I proceed farther, to recall to your memory some of the experiments relative to this subject, made in my country retreat in the course of last summer, and related in my book, principally in p. 155. and 282. together with some further ones I made just

(b) It consists in letting up into the divided glass tube two measures of common air, and afterwards three measures of nitrous air, one after another, shaking the tube in the water trough after each measure of nitrous air, and beginning constantly this motion exactly at the moment the two airs come into contact with each other, or even before they meet, which is still better.

before my setting out for the continent : this recapitulation will make the nature of the experiments just mentioned, and their result, much more easily understood.

The different degrees of salubrity of the atmosphere, as I found it in general in my country house at Southall Green, ten miles from London, from June to September, lay between 103 and 109, that is to say, that of the two measures of air, *viz.* one of common and one of nitrous air, the remaining bulk or column occupied between 103 or 109 sub-divisions in the glass tube.

I was somewhat surprised, when upon my return to town to my former lodgings in Pall Mall Court, I found the common air purer in general in October than I used to find it in the middle of summer in the country ; for on the 22d of October, at nine o'clock in the morning, the weather being fair and frosty, I found, that one measure of common air and one of nitrous air occupied 100 sub-divisions in the glass tube, or exactly one measure. It gave by the Abbé FONTANA's method above mentioned the following result, 184, 208, 304; so that the quantity of both airs destroyed in this trial was exactly one measure and $\frac{96}{100}$ of a measure, or 196 sub-divisions. That very day, at two o'clock in the afternoon (it being then rainy weather) the air was somewhat altered for the worse;

worse; for at that time one measure of common and one of nitrous air occupied 102.

The next day, October 23, it being rainy weather, the state of the atmosphere was the same in the morning as it was the day before in the afternoon: for one measure of common and one of nitrous air occupied 102; and the result of Abbé FONTANA's method was as follows, 184¹, 211, 307, so that the quantity of both airs destroyed amounted to 193.

October the 24th, I again examined the state of the atmosphere at nine o'clock in the morning, the weather being serene; I found it restored to its former goodness; for one measure of common and one of nitrous air occupied 100, or exactly one measure; and the result of Abbé FONTANA's method was 184, 207, 304. At seven o'clock in the evening of the same day the air was again grown worse; for one measure of common and one of nitrous air occupied 103.

October 25th, I examined the air at eleven o'clock in the morning, the sky being cloudy, and found, that one measure of common and one of nitrous air occupied 102. I put it again to the test at eleven o'clock at night, and found, from five different trials, that one measure of it with one of nitrous air occupied 105.

October 26th, the weather being very dark and rainy, I found that one measure of common and one of nitrous air occupied 105.

You know, SIR, that the situation of my lodgings was such, that the back part of the house was contiguous to the garden of Carleton House, which is amply furnished with lofty elm trees, so as to make the garden appear from my windows like a forest. As I had discovered, in the course of last summer, the great power which the leaves of trees possess of improving the atmosphere, by pouring down an invisible shower of purified or dephlogisticated air, during the day-time, in clear weather; I could not forbear ascribing in some measure the purity of the common air of that spot to the happy situation of the place just by so many trees, which had all kept their leaves in full vigour till that time. But I only give this as a conjecture; for I am really sorry that I cannot prove it by a direct experiment. To put out of all controversy such a powerful influence of neighbouring trees upon the circumambient and unconfined air^(c), the air of different and distant

(c) I say *unconfined*, for I am not quite sure that the influence of trees and other vegetables, though in reality very great (as I think I have put beyond all doubt in my book) can be more sensible near trees in the open air than at some distance; in the same manner as some distilled water, poured gradually among an immense mass of common water, would diffuse itself soon through the whole mass equally.

places should be examined at the same time as the air near the trees. I was desirous of doing it, and gave orders to my servant to gather air from different parts of London; but the hurry of business, occasioned by our approaching departure, made him forget or overlook my orders; and for the same reason I myself forgot to perform the task. After all, the best, and perhaps only good way to clear up this matter would be, if several philosophers, each provided with an exactly similar apparatus, and living in different parts of the town, and in different parts of the country, should at the same time put the air of the place of their abode to the test, and afterwards compare their notes. I conceived some flattering hopes before I set out from London, that ere long such eu-diometers, as I have described in my book, will come into the hands of many good philosophers who will take upon them with pleasure to perform this task, as I heard that some of them were already ordered to be made for many persons, among the rest for Dr. HEBERDEN.

Though I have some reason to believe, that the vicinity of the trees did really contribute somewhat to render the air purer at the above mentioned place; yet I believe also, that the frosty weather of itself contributes to purify the atmosphere (perhaps by checking a great many

many causes of corruption of various substances); and that the liveliness we commonly enjoy in frosty weather is in a great measure owing to the superior degree of purity of our common element at that time.

I must now return from this digression to the nature of the sea air.

From what I have already related it appears, that the difference between the best atmospheric air I have yet found and the sea air, as I found it by the first examination upon the spot where chance carried me, is as 91 to 100, the lesser number indicating the best quality. Now, as I found the sea air of such a pure quality so near land, I thought it might, with some degree of probability, be expected, that the common air, at a distance from land, would prove of a still superior quality; for I could scarce believe, that in the first trial, made without choice of place or time, I had just hit upon a time and a place where the sea air is of the first quality.

I would have repeated the same experiment next day, November the 4th, when we were in the middle of the channel between the English coast and Ostend; but the motion of the ship, which was very great, made it impracticable. Not intirely, however, to lose the opportunity which the voyage afforded me, I filled three phials, made on purpose for such use, with

air, when we were in the middle of the sea (the wind blowing pretty hard, and the weather being rainy). I kept these bottles shut till next day, November 5th, when I examined the air confined in them at Ostend, and found it of an inferior quality to that which I had tried on the 3d of November, in the mouth of the Thames; for one measure of it with one of nitrous air occupied, in three different trials, 097. I found the common air at Ostend near as good the same day at ten o'clock in the morning (the weather being cloudy); for one measure of it with one of nitrous air occupied 098. In the afternoon (the weather being very rainy) the common air of the place was become worse, though still of a very good quality; for one measure of it with one of nitrous air occupied 100, or exactly one measure.

About five o'clock in the evening, the weather continuing rainy, and the wind blowing at that time very hard from the sea, I went to the sea-shore on purpose to gather in a phial, fitted for the purpose, the sea air just as it blew towards the land. When I had got it I went immediately home, and put it directly to the nitrous test. I found, by several trials of such good quality, that it was nearly as good as that which I had met with in the mouth of the Thames; for one measure of it with one of nitrous air occupied 094 and 095, whereas

whereas the common air of the inn (as I found by trial at the time) was somewhat of an inferior quality, though still remarkably pure; for one measure of it with one of nitrous occupied 097 in five repeated trials.

As the difference in the quality of the sea air examined on the spot in the mouth of the Thames, November 3d, and that which I gathered in the middle of the sea in rainy and windy weather, was so remarkable, I suspected the reason of this difference to be, that the air, put to the test November 3d, had been exposed during several days to the influence of the sea without any mixture of land air, as it had been remarkably calm all that time; and that the air gathered on the sea in windy weather was mixed with air driven from the land by the wind, and incorporated with the sea air. This suspicion was afterwards strengthened when I found the air gathered at the sea shore, on the evening of November 5th, near as good as that which I gathered on a fair day in the mouth of the Thames, November 3d; for the wind being N.W. the air driven upon the coast was to be considered as true sea air, without any mixture of land air.

But after I had made up my mind about the difference of the above related experiments, a doubt rose in me about a circumstance to which this difference might have been owing, at least in some measure; the circumstance I mean

was this: I had made use of sea water for the experiment on the 3d of November, whereas I had made use of pump water in examining the sea air kept in bottles at Ostend. I thought I had no right to draw any conclusion from the fact till I was convinced that the making this experiment in common or in sea water would make no difference in the result. This consideration made me stay one day longer at Ostend, on purpose to satisfy my mind on this head, lest I should never find another opportunity of doing it. I immediately ordered a pail full of sea water to be brought to my lodging, and made several comparative trials with atmospheric air in common water and in this sea water; but I could not observe any real difference in the result. Thus all degree of suspicion about the difference of the result from this cause was now at an end.

There now only remained some little suspicion that the air gathered on the sea, and kept in phials, might have undergone some alteration; this might have been the case, as I had found in some former experiments made in England, that air kept in bottles was sometimes liable to alterations, which I think is partly to be ascribed to the difficulty of finding bottles so well secured by a ground stopper as to shut out every communication with the external air, and partly, perhaps, to the nature of air, which is not in itself an unalterable substance,

as I have attempted to prove in my book upon Vegetables, p. 107 : in the present case, however, I rather incline to think, that the reason why the air, gathered in rainy and windy weather on the sea, was found of an inferior quality, is to be ascribed chiefly to the land air being driven by the wind upon the sea, and thus to the mixture of both airs.

November the 6th, about nine o'clock in the morning, the weather being cold, windy, and cloudy, I again put the common air of Ostend, which I gathered in my inn, to the test, and found it of near as good a quality as that in the mouth of the Thames; for one measure of it with one of nitrous air occupied $094\frac{1}{2}$ in three repeated trials.

The same morning, about eleven o'clock, the wind blowing very hard from the sea, I went to the shore on purpose to gather some air just as it came from the sea. I found its quality inferior to that which I had examined two hours before, though still superior to any air I have yet found in England; for one measure of it with one of nitrous air occupied 097. The common air, as I found it in my lodging, was at 098. The wind had not shifted much, though I cannot ascertain the exact point from which it then blew. It seems probable from the foregoing experiments, that though in general the sea air

surpasses the land air in purity, yet there are the same inconstancies in its degree of goodness as in the land air. I will not attempt to give any reason for this inconstancy, as I have no experiments to support any; but I think it highly deserving the attention of philosophers to attempt the discovery of this phenomenon.

This experiment being finished, I closed my inquiries at Ostend, and set off for Bruges.

We arrived at dark, and about seven o'clock in the evening I tried the common air of that place, and found it inferior in purity to that of Ostend in above ten experiments: one measure of it with one of nitrous air occupied 105, or thereabout. I had the mortification to find the stoppers of the phials in which I had kept the air of Ostend all loosened, so that I could not make any comparative trial with both airs.

November 8th, I set out for Ghent, where I spent the next day, November 9th. I tried the air of that place about three in the afternoon, and found it better than that of Bruges; for one measure of it with one of nitrous air occupied 103, or thereabout, in several experiments.

As all the following trials with common air are made in the same way as the foregoing, *viz.* by mixing one measure of common with one of nitrous air, I will hereafter only

only mention the numbers of the result, for the sake of abridging the paper by avoiding continual repetitions.

November 12. At Bruffels, I found the air, at seven o'clock in the evening, at $105\frac{1}{2}$.

November 13. I found the air of the lower part of the city at 106, that of the highest part at 104; the weather was rainy and damp. It is a common opinion at Bruffels, that the air in the lower parts of the city is more unwholesome than in the higher parts, and that people of bad constitutions cannot stand it unless they go to live in the higher parts.

November 14. I found the air of the lower and higher parts of the city of the same goodness, each being at 103. The weather was fair and frosty.

November 15. The air was the same as yesterday in both parts of the city; the weather fair and cold.

November 22. I arrived at Antwerp, where I found the air in the evening at $109\frac{1}{2}$; the weather was rainy, damp, and cold.

November 23. I set out for Breda. I filled a phial with air, when I set out from Antwerp at eight o'clock in the morning. I also filled a phial with air on the middle of the heath or common called *De Lange Hey*: the weather was remarkably close and damp. I tried these two airs at night, at Breda; that of Antwerp was found

at 106; that of Breda the same; that of the heath at 105 $\frac{1}{2}$.

November 24. I examined the air at Breda in the morning, about eleven o'clock, the weather being fair, cold, and inclining to frost; it was at 102. At seven o'clock in the evening it was at 103.

November 25. The air at Breda was at 104; the weather rainy and cold.

November 26. The air was in the morning and in the evening 103; the weather very rainy, cold, and stormy.

November 27. I set out from Breda for Rotterdam, and crossed the water at the Moordyke. I tried the air at the Moordyke close to the water, and found it at 101 $\frac{1}{2}$; the weather was fair and cold, but not frosty. This spot is reckoned very healthy; the inhabitants have a sound look, and live to a great age in general.

November 28. I examined the air at Rotterdam, it was at 103; the weather rainy and cold.

November 29. Being at Delph, I gathered some air in the middle of the day, the weather being stormy and rainy. I examined it next day at the Hague, and found it at 103; and that of the Hague 104.

November 30. I examined the air at the Hague, and found it at 104; the weather cold, the wind Northerly.

December

December 1. Being still at the Hague, the weather underwent a sudden and remarkable change. The wind was southerly and stormy; the air was become so warm that upon going out of the house into the street, I felt the same sensation as upon coming from a cold air into a room heated by a German stove. I suspected that this sudden change would alter the constitution of the atmosphere in point of salubrity. Having no time to make any experiment, I contented myself with filling some phials with this air, and sending my servant to Schevelingen to gather some air close to the sea.

December 2. The wind and weather remained the same as yesterday. In the evening, about eight o'clock, when FAHRENHEIT's thermometer stood at 54° , I put the common air to the nitrous test, and found it at 116; the air gathered the day before at 117; and that gathered close to the sea at 115. As I had never found the common air near so bad, I had some apprehension that my eudiometer was out of order, or that something was the matter with the nitrous air. I made therefore fresh nitrous air, and repeated the experiment many times, but the result was nearly the same. In the mean time, I had the following accidental meeting. The father of the landlady of the house having been informed by the servant, that I was about some extraordinary pursuit, of which

he could have no conception, was led to come and see what I did. He had scarce been a minute with me but I perceived he laboured under a severe asthma. He explained his case to me, knowing me to be a physician, and told me, *that he had passed these two days very uncomfortably, finding the air so uncommonly heavy that he could scarce draw his breath:* which convinced me, that the element was in reality become of an inferior quality.

December 4. Being at Amsterdam, I found the air of that place at 103; the weather being rainy, windy, and cold.

December 5. The air was at 102; the weather nearly as yesterday.

December 10. I returned to Rotterdam, and found the air at 101; the weather rainy.

In the beginning of last year they made an end of draining a large meer (about half the size of the Haerlemmer Meer) situated in the neighbourhood of Rotterdam, which was turfed out in former ages. It was now laid into arable land, and turned out to be very fruitful. When this land was quite cleared of the water, an uncommon epidemical disease broke out in all the places situated upon the borders of this lake; it began about August, and abated when the winter season set in. This distemper broke out a-fresh last summer, and was

now again upon its decline. It carried off a great number of inhabitants. It appeared chiefly under the habit of an irregular intermittent, a bilious remitting, and a putrid fever. There was scarce a single house to be found in which there were not some persons sick. The villages at a quarter of a league distance from the former lake were free from it. This distemper was ascribed to the putrid exhalations of this newly uncovered land; which exhalations were very offensive to the smell. This was so much the more probable, as the disease abated when the stench, checked by the cold, abated. I tried the air of this former lake upon the spot, in company with my learned friend Dr. DE MONCHY, professor of physic, and found it as good as that of Rotterdam; but there was a great deal of wind that day, and no perceptible stench. However, Dr. BICKER, an eminent physician of that city, got me a phial filled with air of this lake, which he took from a spot where he still perceived some of the former bad smell. This air proved to be in reality of an inferior quality to that of the city.

December 12. Being in the middle of the water between Dort and the Moordyke, I found the air upon what is called Holland's Diep of an inferior quality, the weather being remarkably dark, rainy, and windy; it was at 109.

December 13. Being returned to Breda I found the air of that place at 109 in the morning, the weather continuing as it was yesterday. In the afternoon it was somewhat better, *viz.* at $106\frac{1}{2}$, the weather having cleared up.

December 16. Having returned to Antwerp, I found the air of the lower part of that city at 105; and that of the higher part at 104, the weather being rainy and temperate.

December 17. The air at Antwerp was 107, the weather continuing to be nearly as it was the day before.

December 19. Being returned to Brussels, I found the air at 109, the weather being rainy, windy, and rather warm.

December 21. I found the air at Brussels at 106, the weather being dry and cold.

December 22. The air of Brussels was the same as yesterday; the weather was nearly the same also.

December 23. I arrived at Mons, and found the air of that place at 104; the weather rainy and cold.

December 24. Being near Bouchain, I found the air at $104\frac{1}{2}$; the weather was cloudy and cold.

December 25. I tried the air at Peronne, and found it at $102\frac{1}{2}$; the weather was frosty.

December

December 26. Being at Cuvilli, a village four leagues from Roye, I examined the air of that place, and that which I had gathered on the road about twelve o'clock in the day-time; I found them both at 103; the weather was frosty.

December 27. I examined the air at Senlis, and that which I had gathered in the middle of the day on the road; both were at $102\frac{1}{2}$; the weather continued frosty.

December 29. Being at Paris, I found the air in that capital at 103; the weather frosty.

1780, January 8, the air of Paris was at 100; the weather very frosty.

January 13. The air of Paris was 98; it froze very hard.

This paper being already too long, I will only add some few deductions, which seem to follow of themselves from the above related experiments.

It appears from these experiments, that the air at sea and close to it is in general purer and fitter for animal life than the air on the land, though it seems to be subject to the same inconstancy in its degree of purity with that of the land; so that we may now with more confidence send our patients, labouring under consumptive disorders, to the sea, or at least to places situated close to the sea, which have no marshes in their neighbourhood.

It seems also probable, that the air will be found in general much purer far from the land than near the shore, the former being never subject to be mixed with land air.

It appears also, that the air in frosty weather is in general wholesomer than it is in winter when it does not freeze; and that uncommon warm weather, happening in the winter season, is apt to render the atmosphere very unwholesome: the reason of which I apprehend to be, that the frost totally checks that general tendency to corruption, which being revived by warmth again increases the infection of the common air, which at that time is so much the greater, because the plants (which are deprived of their leaves in winter) have no power in them to counteract it.

It seems also probable, that those countries which are, by their local situation, exposed to noxious exhalations, are in general much wholesomer in the winter; and that it is much safer to cross such countries in summer time when it is windy weather than in a calm, &c.

How far these deductions are founded upon experience may appear by applying them to such places as they may be found to have a relation to.

My old friend Dr. DAMMAN, an eminent physician and professor royal in midwifry at Ghent, told me, that
when

when he was formerly a practitioner at Ostend, during seven years, he found the people there remarkably healthy; that nothing was rarer there than to see a patient labouring under a consumption or asthma, a malignant, putrid, or spotted fever; that the disease to which they are the most subject, is a regular intermittent fever in autumn, when sudden transitions from hot to cold weather happen.

People are in general very healthy at Gibraltar, though there are very few trees near that place; which, I think, is owing to the purity of the air, arising from the neighbourhood of the sea.

Most small islands are very healthy.

At Malta people are little subject to diseases, and live to a very advanced age.

